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before the first phase of the season's growth is indicated not only by the full number of cells, but also by the presence usually of at least the normal number of needles (*loc. cit.* p. 429).

There are a number of trees of *Picea excelsa* on the campus of West Virginia University that were transplanted in 1899, several weeks earlier in the season than the Bloomington trees. While the size of the cells is not nearly proportional to that of the needles, the number formed during the spring following transplanting falls somewhat behind. The numbers in an average case are as follows:

	1898	1899	1900
Epidermis. ....	122	114	140
Outer parenchyma layer..	91	80	106
Endodermis.....	22	18	25

The whole question of the growth of the needle will reward more experimental study, and Meissner's interesting paper should stimulate it.

The accompanying photograph is of the plants described by me in 1898, and will show the concomitant dwarfing of stems and needles better perhaps than tables of measurements did.—EDWIN BINGHAM COPELAND, *Monroe, Wis.*

## THE INSTABILITY OF THE ROCHESTER NOMENCLATURE.

SINCE the publication of my article in the BOTANICAL GAZETTE for last March<sup>1</sup> three communications have appeared which discuss certain details there presented. Two of these letters, from Mr. C. L. Pollard<sup>2</sup> and from Professor L. M. Underwood,<sup>3</sup> question the significance of facts presented by me in parallel columns; while a third letter, from Professor N. L. Britton,<sup>4</sup> presents at more length than was done by me the practical reasons why the first species of a complex genus should not necessarily be treated as the type. These articles bearing directly upon the vital question of stability in botanical nomenclature must leave the general reader in some doubt as to the exact force of certain arguments I have already discussed. It may not be out of place, therefore, to ask for a brief consideration of the points thus newly emphasized.

<sup>1</sup>BOT. GAZ. 31:183. 1901.

<sup>3</sup>*Ibid.* 31:365. 1901.

<sup>2</sup>*Ibid.* 31:285. 1901.

<sup>4</sup>Science 13:588. 1901.

Mr. Pollard quotes from my article certain sentences embodying conclusions which he characterizes as misleading. The conclusions thus referred to were to the effect that in 1900 Professor Underwood, following logically and consistently the Rochester Code, used 25 per cent. of names different from those used by him in 1896, when he likewise asserted that he was following the Rochester Code. Mr. Pollard's contention is that this fact does not afford a proper basis for criticism of the Rochester Code, because most of the changes made by Professor Underwood represent very recent segregations among the ferns, comparable with the divisions made within three years in the genus *Antennaria*. Yet it does not seem to have occurred to Mr. Pollard that, in omitting the footnote to the paragraph quoted, he is possibly misleading that large body of readers who habitually neglect to verify quotations and references. Had he carefully read and quoted the two sentences as they were originally published with the footnote: "The true ferns alone are here considered, and the genus *Botrychium* is purposely omitted, since that genus has been subdivided by Professor Underwood to such an extent that comparative figures would have little definite significance," he would have found it unnecessary to inquire whether I do not "recognize the necessity for occasional segregations." Every working systematist must recognize such necessity; but the segregation of perplexing polymorphous types is a very different thing from the raking up of older and more or less obscure appellations for plants the names of which have long been established. The latter is a nomenclatorial matter, the former botanical; and if anyone has read into my previous remarks the least opposition to such division of confused groups as shall lead to a clearer understanding of the forms, he has found that which I in no way intended.

But most of the ferns under discussion cannot be placed by Mr. Pollard under the same category as the recent segregates of *Antennaria* (and likewise *Botrychium*), for *they do not represent new points of view unknown in 1896*. *Matteuccia* is a name given in 1866 to Willdenow's *Struthiopteris*, which was segregated in 1809 from *Osmunda*; our *Dennstaedtia* was separated from *Dicksonia* in 1857; *Filix* was published by Adanson in his well known *Famille des Plantes* in 1763; *Phyllitis* was re-distinguished in 1844; and it is certainly not a new idea to treat species of the complex genus *Aspidium* under the name *Polystichum*. Neither are *Phegopteris Robertiana* and *Notholaena dealbata* now treated as species for the first time. The former was published

by Hoffmann in 1795 as *Polypodium Robertianum*, the latter by Pursh in 1814 as *Cheilanthes dealbata*. In 1896 Professor Underwood had already published five editions of a work on North American ferns, and he was consequently in a position to know of the earlier conceptions and treatments of these plants. At that time he was publishing according to his interpretation of the Rochester Code, and the names thus published, we were assured, were those which would stand. Yet after having established (as he supposed) these names according to strict priority principles he has found occasion to alter 25 per cent. of them. Has his application of the Rochester Code brought uniformity?

In his "Open Letter" Professor Underwood shows very conclusively that "when its bald statements are unqualified," "the deadly parallel" "seems to mean more than the facts will warrant." For, avowedly with the purpose of showing that "the personal preference hit-or-miss system of Kew and Berlin" is bringing us unwarranted confusion in plant names, he places in parallel columns twenty-one fern names which appeared in the *first* edition of Gray's *Manual* and the twenty-one more or less dissimilar names for the same plants in the *sixth* edition. By the thoughtless reader, with "its bald statements . . . unqualified," this comparison might seem completely to dispose of my criticism of the recent changes of names made by Professor Underwood. Yet it should be borne in mind that such comparisons may "mean more than the facts will warrant." Professor Underwood implies that the application of the combined Berlin and Kew rules is responsible for the changes between the first and the sixth editions of the *Manual*. Can it be that he "forgot" that the *first* edition was issued in 1848, the *fifth* edition (Pteridophyta by D. C. Eaton) in May, 1867, and the *sixth* edition in 1889, *all before the Berlin rules were formulated in 1897?*

It was three months after the fifth edition of the *Manual* went to press that the Paris convention of 1867 was held and the DeCandolle Code was drawn up. Consequently the sixth edition (1889), by Watson and Coulter, was the only one published after the adoption of the Paris Code of 1867. Furthermore, since even this edition was published eight years before the rules of the Berlin botanists, a comparison of the first edition (1848) and this sixth edition (1889) is no very logical proof that the Berlin rule of 1897 is "unseaworthy."

The fact is, that the names in no edition of Gray's *Manual* have been based on the combination of the Berlin rule for genera and the so-called Kew rule for species, which was recently advocated by me.

This simple and definite method of obtaining a uniform system of names has been carefully tested, however, during the past half decade, and it has appealed as practical to many besides the "trans-Carlins."

Let us now consider, on the other hand, the points raised by Professor Britton. The first rule of the Rochester Code reads: "Priority of publication is to be regarded as the fundamental principle of botanical nomenclature." The fifth rule that "*Publication of a genus consists only (1) in the distribution of a printed description of the genus named; (2) in the publication of the name of the genus and the citation of one or more previously published species as examples or types of the genus, with or without a diagnosis*" [italics ours]. A recommendation adopted by the reformers at Madison reads: "In determining the name of a genus or species to which two or more names have been given by an author in the same volume or on the same page of a volume precedence shall decide."

By Professor Underwood, as already emphasized in the March GAZETTE, this principle of strict priority has been applied in the determination of the type of a complex genus. Professor E. L. Greene, likewise, has made important changes based upon its rigid enforcement. Professor Britton, too, has more than once maintained that priority of place (precedence) is final, saying, in regard to the now famous case of Buda and Tissa: "I accepted *Tissa* rather than *Buda* for the simple reason that it *stands first on the page* in Adanson's 'Familles.' *That is priority, I am sure*" [italics ours].<sup>5</sup> He has further said that this principle (priority of position) will be accepted by those "who have recognized the necessity of adopting methods of procedure which will render the system of nomenclature stable."<sup>6</sup> And again, "the number of cases in which change is desirable by reason of priority of place is not great."<sup>7</sup> Thus, in 1890, Professor Britton clearly defined what he meant by priority. In 1892, "after a very careful consideration," the Rochester Code was formulated; and in 1893, after plenty of time for further deliberation on its fundamental principle, the code was augmented by the recommendation above quoted.

Priority has been talked of until the subject has become a tedious one; and that it is the ruling principle of the reformers has been so often avowed as to become axiomatic. It is not surprising, then, to find, in the *Illustrated Flora* in 1896, the statement that "its [priority's]

<sup>5</sup>Jour. Bot. 28: 295. 1890.

<sup>6</sup>Jour. Bot. 28: 371. 1890.

<sup>7</sup>Jour. Bot. 28: 372. 1890.

adoption is the only practicable way of securing stability to the original names." This idea has permeated the writings of the reformers, and they have over and over again asserted in books and papers for amateurs and "the younger generation" that the names they advocate are alone the ones which can stand.

That the principle of strict priority (the fundamental law of the Rochester Code) has been many times ignored by those who claim to follow it was sufficiently emphasized in the *MARCH GAZETTE*. Indeed, when Professor Greene pointed out,<sup>8</sup> in 1896, that the principle of priority demanded that the first species of a complex genus be taken as the type, he was merely showing the logical outcome of the principle. When again, in 1899, Professor Underwood followed the same interpretation in his *Review of the Genera of Ferns*,<sup>9</sup> he was merely following conscientiously the fundamental principle of the Rochester Code, and the law so often vigorously defended by Professor Britton, who "accepted Tissa rather than Buda for the simple reason that it stands first on the page in Adanson's 'Familles,'" saying, "That is priority, I am sure."

After such clear definition of the principle of priority by Professors Britton, Greene, and Underwood, we are now amazed to see from Professor Britton's pen a convincing argument against the uniform selection of the first species as the type of a complex genus. Where shall we look for that long-promised "uniformity" when, in writing of the principle as logically followed by Professor Underwood and more than once by Professor Greene and by Mr. Thomas Howell, Professor Britton now says, "Inasmuch as a great many genera have at their first publication been made to include more than one species, and in a large number of instances some of these, often the first in position, have been used by subsequent authors as the types of additional genera, this latter-day proposition affects an enormously greater number of cases than those which fall properly under the operation of the rule" ?<sup>10</sup> And when, continuing his argument against the necessary acceptance of the first species as the generic type, he says "*it is, therefore, clear that there is nothing logical in the proposed extension of the principle*" [italics ours],<sup>11</sup> does he not directly contradict the conclusions of Professor Greene and Professor Underwood? Furthermore, how are such views reconciled with the fundamental principle

<sup>8</sup> *Pittonia* 3: 128. 1896.

<sup>10</sup> *Science* 13: 588. 1901.

<sup>9</sup> *Mem. Torr. Bot. Club* 6: 247. 1899.

<sup>11</sup> *Science* 13: 588. 1901.

of the Rochester Code, and with the former statements of Professor Britton himself, who in 1890 emphatically argued of "priority of place," that "that is priority, I am sure"?

When the Rochester Code and its Madison amendments were put forward we were repeatedly told that their *raison d'être* was to establish our plant-names upon a permanent basis. Therefore when the *Check List* was published it was naturally supposed that the names there included were final. At great inconvenience to all branches of American botany we have been forced, consequently, to stumble through a most perplexing tangle of ever-changing "permanent" names. If all this confusion and inconvenience were leading us by the shortest course—or by any course—to stability, no one but the short-sighted would complain. It is true that, in spite of their frequent changes from one policy to another, and notwithstanding the utter abandon with which they trifle with the most important principles, the reformers still claim to be bringing us uniformity. How can this be? Professor Underwood stands firmly for priority of place, claiming that the first species in the genus must be taken as the type. Professor Greene has taken the same ground, though openly arguing against some other principles of the Rochester Code. Now, after agitating for years the principle of strict priority and more than once defining his understanding of the term, Professor Britton has published an argument squarely opposed to the uniform acceptance of the first species of a complex genus as the type. Can it be, then, that with such hopeless diversity of opinion on the part of two leading reformers at the same university, they still have sufficient sense of humor to tell us that they are establishing uniformity? If Professor Underwood, following consistently the spirit and text of the Rochester Code, believes in changing "99 per cent." of names; and Professor Britton, abandoning the fundamental principle for which he has so long argued, changes names on a radically different basis, will they not give us systems of names more and more hopelessly unlike?

Professor Britton declares the position taken by Professor Underwood a "latter-day proposition," thus implying that the question of generic types was not seriously considered at Rochester and at Madison. Yet if the Rochester Code is the result of a "very careful consideration," how can this vital matter have been overlooked? However, the Rochester Code tells us that "priority of publication is to be regarded as the fundamental principle of botanical nomenclature,"

and Professor Britton has said "it is perfectly clear that as long as we allow ourselves a choice of names *in any way*, so long will authors differ in their acceptance and the settling of this important matter be deferred" [*italics ours*].<sup>12</sup> From these assertions the only logical conclusion is that doubtful cases are to be referred to the principle of priority. The decision between Buda and Tissa, and the determination of the generic type both "allow . . . a choice of names," and according to the first principle of the Rochester Code priority alone should settle them. The question of Buda and Tissa is one of decision between two generic names for the same plant; the question of the generic type asks which of two or more plants shall bear a given generic name. When these questions have such fundamental similarity, how can a reformer maintain that priority is to decide in one case and not in the other?

When Professor Britton now maintains that the first species of a complex genus is not necessarily the type, and that in such cases the Rochester code allows us to cling to the traditional genera, he at once places himself on record as likewise opposed to the fourth rule of the Rochester Code. This rule reads:

IV. HOMONYMS.—The publication of a generic name or a binomial invalidates the use of the same name for any subsequently published genus or species respectively.

Let us look, for example, at the case of *Mimosa*, a name which Professor Britton and other reformers use in its traditional sense. The Linnæan *Mimosa*, published in the *Species Plantarum*, contained 39 species, the first on page 516, the last on page 523. Of these species only six are now retained in the genus as finally defined by Bentham in 1875 and now generally accepted. The first species now recognized in the Benthamian genus *Mimosa* is *M. viva*, no. 11 of the *Species Plantarum*. This species is the last one on page 517, and on that page it is preceded by six species, and on page 516 by four species, all of which are now treated as members of other genera. If Tissa has priority over Buda "for the simple reason that it stands first on the page," then surely by the same logic *Mimosa bigemina* and the other five species on page 517 have priority over *M. viva*. And still more clearly *M. viva* is preceded by *M. Lebbeck* (now treated as an *Albizzia*) and the other species on page 516. *Mimosa* was published on this page (516) of the *Species Plantarum* as the name of a

<sup>12</sup> Jour. Bot. 28: 372. 1890.



genus; then *M. Lebbeck*, *M. Inga*, *M. fagifolia*, and *M. nodosa* were published, not only with citations of their previous places of publication, but with brief diagnoses as well. Consequently the publication of the generic name *Mimosa* and the first of the species, *M. Lebbeck*, satisfies the Rochester requirement for the publication of a genus (see above). Furthermore, the species was treated as a *Mimosa* subsequent to its original publication in 1753, and it was first removed from the genus by Willdenow in 1806. Therefore, when the name *Mimosa* in the Benthamian sense is applied by Professor Britton to *M. viva* (and its congeners) while on a preceding page the same generic name had been applied to *M. Lebbeck*, a plant of different generic affinity, he uses a name which is a homonym.

Two fundamental principles of the Rochester Code are thus deserted by the chairman of the Rochester committee on nomenclature, while strongly defended by only one of the original members. And now from Nebraska, which has not long been notable as the seat of ultra-conservatism, comes the announcement, in regard to the recent work of the only consistent member of the original committee, that "it shakes one's faith in the immutability of things to find old friends under unfamiliar names."<sup>13</sup>

The reformers wedded themselves to the principle of strict priority. At the start they ignored the reasoning of those who foresaw where it would lead them. They rushed headlong and short-sighted into the scramble for hidden and obscure names. For a short time they worked harmoniously. Then came misunderstanding and lack of unity. One member of the nomenclature committee disclaims "that there is any obligation to elevate varietal names to specific rank when the [aggregate] varieties themselves are thus promoted."<sup>14</sup> In so doing he opposes the rule of the *Check List* committee which says "that the original name is to be maintained whether published as species, subspecies, or variety." Another member stands for absolute priority and takes a course in which Professor Britton finds "nothing logical." The chairman of the committee now proclaims that priority of place (precedence) is not necessarily final. In so doing he opposes not only the first, but the fourth of the Rochester principles. To every logical and fair-minded scientist who will take the trouble to consider the question calmly and judicially, this woeful diversity in the practices of the committee must be apparent. No one can say

<sup>13</sup> Science 13 : 833. 1901.

<sup>14</sup> GREENE, E. L., *Pittonia* 4 : 253. 1901.

today where individual members of the *Check List* committee will stand tomorrow. They have forgotten or ignored the fact that the Rochester Code was to give us permanent names. They have made of it a "personal preference hit-or-miss system." In the words of one of their number, they are "openly at war with their own rules." Is this uniformity? Is this "the day of law"? Is this the high road to a stable nomenclature?

Do we sincerely want uniformity, or do we prefer the tangled results of individual interpretation? If the former ideal still appeals to us, why not abandon this restless pursuit of the will-o'-the-wisp? Why not honestly test the combined Berlin rule for genera and Kew rule for species? None of their opponents have given them a fair trial. Until they do can they really judge of their merits?—M. L. FERNALD, *Gray Herbarium*, June, 1901.

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### FLOWER VISITS OF OLIGOTROPIC BEES. III.

AMONG the oligotropic bees mentioned in BOTANICAL GAZETTE 28: 36, 215, and 30: 130, should be included: *Andrena krigiana*, which collects its pollen from *Krigia amplexicaulis*; *Entechnia taurea*, which is an oligotropic visitor of *Ipomoea pandurata*: and *Anthedon compta*, which gets its pollen exclusively from *Oenothera biennis*.

Species of *Melissodes*, which usually collect the fine pollen of *Compositae*, have their scopae dense and quite closely plumose. On the other hand, *Emphor*, *Xenoglossa* and *Entechnia*, which collect the large pollen grains of *Hibiscus lasiocarpus*, *Cucurbita pepo*, and *Ipomoea pandurata*, have their scopae quite loose and thinly plumose. The close relationship of *Anthedon* to *Melissodes*, and the fact that the male has quite plumose hairs on his hind tibiae, show that the scopae of the female have recently lost the barbs and have come to be composed of simple bristles. I have wondered why this was so, and have expected to find some peculiarity in the pollen which the bee collects. Now in *Oenothera biennis* the pollen grains are large, trilobed, and connected by cobwebby threads. This condition of the pollen makes the barbs unnecessary if they would not greatly interfere with the collection of this kind of pollen.

*Andrena nasonii*, mentioned in the first list, is not oligotropic.

In the *Fertilization of Flowers*, p. 570, in discussing the effect of conspicuousness of flowers in inducing insect visits, Müller says: